

Framework For Prioritizing Regulated River Rehabilitation

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0173: Framework For Prioritizing Regulated River Rehabilitation

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Goals are to quantify the linkages between hydrologic and geomorphic links governing spawning habitat to assess potential persistence of restored functions on a river prior to rehabilitation. Methods use flow regime components and associated geomorphic descriptors. Aim is to examine post-dam flows with respect to reference hydrograph (i.e. pre-dam) that is assumed to grant functional flow components. Conditions are that post-impact fines should mobilize fines from gravel interstices and from over-egg pockets during incubation and emergence, should provide full mobility of gravel to form riffles and to maintain spatial variability before spawning; should not further armor the river bed, and should not increase scour of egg pockets during incubation. Then, based on these results, the authors will select river with greatest potential for recovery among 3 rivers (Mokelumne, Cosumnes, Yuba). Functionality of flow components will be tested through new data collection and analysis of existing data. Survey cross sections of all spawning riffles downstream of dams on rivers. Calculate shear stress, armoring, geometry, duration of transport regimes of the 3 rivers for comparison.

HYPOTHESIS: Ultimate RESEARCH QUESTION should be simplified to: Do IHA scores correspond to geomorphic conditions? Methods use flow regime components and associated geomorphic descriptors. Aim is to examine post-dam flows with respect to

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reference hydrograph (i.e. pre-dam) that is assumed to grant functional flow components. Conditions are that post-impact fines should mobilize fines from gravel interstices and from over-egg pockets during incubation and emergence, should provide full mobility of gravel to form riffles and to maintain spatial variability before spawning; should not further armor the river bed, and should not increase scour of egg pockets during incubation. Then, select river with greatest potential for recovery among 3 rivers (Mokelumne, Cosumnes, Yuba). Functionality of flow components will be tested through new data collection and analysis of existing data. Survey cross sections of all spawning riffles downstream of dams on rivers. Calculate shear stress, armoring, geometry, duration of transport regimes of the 3 rivers for comparison. Reviewers did a great job!

Additional Comments:

PIs are well qualified. Budget looks reasonable. CONCERNS: Proposal on the whole is vague, and VERY-salmon centric. There was no attempt to link this proposal to any element of river restoration, or to any other stage of salmon except the egg stage. It seems dangerous to key restoration to one life stage of one species, even though it's a threatened species. Another potential problem is that the proposal ignores chemical conditions that may affect survival of eggs. The reviewers suggested an alternate hypothesis for this project. Do IHA scores correspond to geomorphic conditions? Another assumption is that pre-impact river conditions provide a good basis for restoring river health. It is unrealistic to assume that the river can be returned to pre-impact conditions. A better alternative is to determine what Pre-Impact relationships are between shear/stress/flow regime/substrate, then mimic this relationship in current situation. It was not clear how the method will be evaluated. How will its success be determined? Are there rivers where alternative flows have been used where the method could be used and its ability to rank sites with known outcomes be compared?

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Framework for prioritizing regulated river rehabilitation

The panel noted that the proposal was based solely on using salmon spawning condition as a criterion for prioritizing rehabilitation and that in reality, salmon are responsive to many other parameters. It was not clear how the proposed method would be validated, if at all. It was recommended that this study would have been stronger if its focus was only on flow regime and geomorphic measure analysis, not on development of a prioritization tool.

Final Ranking: Adequate

Technical Review #1

proposal title: Framework For Prioritizing Regulated River Rehabilitation

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals and objectives are timely and important. For successful and replicable river restorations it is crucial to understand the linkage between hydrology, geomorphology, and ecology. Specifically, the link between hydrology, geomorphology, and fall run Chinook salmon spawning is necessary to be able to target specific restoration actions that will increase spawning success.</p> <p>The goals are properly based on the idea that restoration relies on understanding processes.</p> <p>The Project Purpose section is very broad and does not focus the proposal on the specific project. Although this proposal is actually very specific to a study of shear stress in relation to flow regime and geomorphology only as it applies to fall run Chinook salmon spawning success, the proposal is worded to suggest it is addressing a much broader issue when in truth it is addressing only a small issues that fits within the much broader issue. For example, the phrase 'ecological recovery' is used frequently where the phrase 'fall run Chinook salmon</p>
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	<p>spawning habitat restoration' or at least 'Chinook spawning recovery' would be more appropriate.</p> <p>The hypotheses seem obvious and do not specifically lead to tests. For example hypothesis 1 supposedly will establish that a science based method to enable quantitative assessment of the long-term restoration potential of stream channels exists. It seems to me that this is the basis of all hydrogeomorphology/stream ecology studies. Does this really need to be stated as a hypothesis? Is this single study going to put that huge question to rest?</p> <p>The second set of hypotheses reads like statements. It seems very likely that these statements actually refer to relations that are already known, if not entirely quantified. These seem more like the basis of a study approach rather than hypotheses that will be tested. Perhaps this whole section could be condensed into a single question such as: do IHA scores correspond to geomorphic conditions?</p> <p>The third set of hypotheses seems circular. Each sub-hypothesis could simply be stated as: we expect similar conditions to be similar. Perhaps more detail would clarify what is intended. What is geometric similarity? What is comparable spatial variability?</p>
Rating	fair

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Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>In general, the study is well justified because the link between geomorphology, hydrology, and ecology is definitely in need of better quantification. Specifically, the study clearly targets understanding how altered flow regimes interact with substrate and channel morphology to either reduce or increase fall run Chinook salmon spawning success. However, the justification is out of sync with the Project Purpose section. The Project Purpose section is very broad. Perhaps the Project Purpose section should be removed altogether and the Justification section could be moved to the beginning so that the reader will immediately be introduced to the rationale for the study.</p>
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The approach is adequate to provide decision makers with information on differences between the three river reaches. Quantification of the link between flow regime, geomorphology, and fall run Chinook salmon spawning habitat will help managers understand what hydrologic and geomorphic features are critical and this will have implications for all Chinook salmon and perhaps all salmonid rivers worldwide.</p> <p>The approach has two theoretical downfalls. First is</p>
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the assumption that the pre-impact flow regime is 'best' for fall run Chinook salmon spawning. Every river has a unique natural flow regime. How can each different regime also be best for fall run Chinook salmon spawning? Certainly pre-impact flow regimes were more favorable than many modern impacted flow regimes but it is doubtful than any natural flow regime truly maximized fall run Chinook salmon spawning and definitely not in every year, because every annual flow regime is also unique. This is a question of objectives. Is the sole objective of the researchers to maximize only fall run Chinook salmon spawning success? If so then the approach should simply seek to study each of the factors that affects spawning success and then study each river to determine how to manipulate the flow regime and geomorphology appropriately. The pre-impact flow regime has no bearing on this objective.

More importantly, the pre-impact flow regime is meaningless if it is removed from the pre-impact context that includes pre-impact watershed conditions, pre-impact climate, pre-impact valley features, and pre-impact vegetation. It is inappropriate to apply the pre-impact flow regime to the post impact river. If the pre-impact flow regime was restored to each river reach (which of course is technologically and politically impossible) it would be highly unlikely to restore fall run Chinook salmon spawning habitat, even if the high flows could be contained within the modern channel.

If pre-impact data on all the study variables are available, then the proposed study could determine the pre-impact shear stress/flow regime/substrate relations and then determine what conditions would result in similar relations in the modern impacted river reaches. Alternatively studies could be conducted on pristine rivers with fall run Chinook salmon to determine pre-impact shear stress/geomorphology/hydrology relations. Otherwise,

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	<p>the appropriate approach is to simply rely on the relations that are already known to affect fall run Chinook salmon spawning and study ways to improve conditions within each river reach. There is no need to refer to the pre-impact flow regime.</p> <p>Second, fine sediment is not ultimately an instream issue. The proposal seems to imply that fine sediment is and has always been controlled by daily flows that keep gravels clean (free of fine sediment). This is not the case. Fine sediment in rivers is anthropogenic. The appropriate way to reduce fine sediment from spawning gravels is to keep it from entering the river channel by revegetating the watershed and, where revegetation is not possible, trapping the eroded topsoil before it reaches the river. Thus, it would be more appropriate to view management activities that flush silt from instream gravel as a necessary stop-gap measure until watersheds are able to substantially reduce erosion. Prior to human impacts, fine sediment would have only been a major impact after major geomorphic events such as landslides or periods of major climate/geomorphic change.</p>
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The approach of quantifying relations between shear stress, channel morphology, channel substrate, and flow regime in relation to fall run Chinook spawning habitat is fully documented and technically feasible. The likelihood of success is high. The authors are experienced and will likely easily accomplish this study.</p>
Rating	very good

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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The field work plan is appropriate. The field data will be the crux of the study.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Products of value will likely be produced by the prolific researchers. The study will add a small piece to the overall picture of relations between hydrology, geomorphology, and ecology, but it will be a valuable piece from the standpoint of fall run Chinook salmon conservation. Interpretable outcomes are likely because of the comparative approach (i.e., three rivers with highly variable conditions will be studied with the same methodology) and the focus on process, that is, the relation between shear stress, channel morphology, and channel substrate composition.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	
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	The track record of the authors is impressive. The team is well qualified and has access to adequate infrastructure.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is modest but is likely adequate for the specific objective of identifying relations between shear stress, flow regime, geomorphology, and fall run Chinook salmon spawning habitat.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	I would like to give this proposal a very high rating. The specific goals and objectives are appropriate and timely and important. The proposal is appropriately focused on process rather than on static habitat features. However, important theoretical issues (discussed above) and vague hypotheses are troubling. If this proposal could be rewritten to be more focused on the specific study it would be greatly improved and possibly result in a better study. The project should focus entirely on instream processes and not be concerned with pre-impact flow regimes that have no relevance to the modern river reaches that are being studied. In this same vein, the researchers should recognize that not all of the modern processes or issues were present historically. For example, the source of fine sediment is much different today than pre-impact and is currently a chronic problem whereas historically it was a catastrophic problem. In short, although the level of detail is high in spots, the
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	proposal as a whole is vague because sections do not easily fit together in the reader's mind and hypotheses do not explicitly establish what products will specifically be generated or exactly how these products will fit within the broader goal of making the link between hydrology, geomorphology, and ecology, which is discussed at great length.
Rating	good

Technical Review #2

proposal title: Framework For Prioritizing Regulated River Rehabilitation

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals and objectives and hypotheses are clearly stated and they are internally consistent. I think the general idea is important since it offers a means of prioritizing river restoration actions. However, this is a "salmo-centric" view of restoration that does not address any other aspects of river restoration.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The proposed study is supported by existing literature that defines certain hydrogeomorphic-ecologic links that govern spawning habitat quality and success. However, there is no attempt within the proposed project to test any of the hypotheses with concurrent ecological data collection. The entire project relies on the assumed ecologic linkages, and as such the results of the empirically-based analyses are not actually testable.
Rating	

Technical Review #2

	good
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Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The proposed approach is well designed for meeting the stated goals of the project. Similar data and analyses are proposed for each of the 3 rivers, each of which has been hydrologically altered to a different degree. The approach to field data collection is feasible. I am, however, somewhat concerned that the hydraulic analyses that will be used to quantify the shear stresses that define the threshold conditions for the various ecological functions, will be based on normal-depth methods, especially since the analysed sites will be located at riffles. For this reason I am concerned about the utility of the results for decision making purposes. At a minimum I would suggest that the proposal be ammended to include at least one-dimensional (e.g. HEC-RAS) hydraulic modeling. Based on the senior author's past research in this field in which he has strongly argued for the use of multi-dimensional hydrodynamic modeling, I'm somewhat suprised by the decison to use normal-depth hydraulics as the basis for his analyses of sediment transport related ecologic functions.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach has been well documented, but I am
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	concerned that the use of normal-depth hydraulics will limit the likelihood of successs. Based on the authors' previous experience, the project scale is well within their abilities. A reduction in the number of sites analysed may allow them to put more effort into the hydraulic analyses for the individual sites.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is not proposed and would not be appropriate for this project.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Within the limitations imposed by both the reliance on assumed geomorphological-ecological links and limited site-specific hydraulic analyses, I believe products of value will be developed from this project. The availability of, and contribution to, data management systems is relevant and has been considered.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	I believe that the PI has a well documented track record of success in this field, and that his supporting staff are well qualified to both collect the appropriate data and do the required analyses. Available infrastructure appears adequate to meet their needs, and access to field sites has been secured.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	For the level of analyses that are being conducted I think the budgets for the individual sites are reasonable and adequate. I do, however, recommend that the number of sites be reduced in the interest of applying more of the budget to conduct a more rigorous site-specific hydraulic analysis.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	I think this proposal presents a good idea, but it also presents a very limited salmo-centric view of river restoration. There is no attempt to relate the
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	hydrogeomorphic-ecological linkage required for successful spawning to any other elements of river restoration, and as such, any prioritization based on the results of this project is likely to be of limited use. The authors are certainly capable of conducting the proposed research and the budgets appear reasonable. I would prefer to see the number of research sites reduced on each river so that more effort can be put into additional hydraulic analysis, that is the foundation for the proposed methodology.
Rating	good

